LETTERS TO THE EDITOR.

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Escape of Gases from Atmospheres.

I ASK for space to reply to Mr. Cook's letter in last week's NATURE.

There are two ways in which the rate at which gases escape from atmospheres may conceivably be investigated, viz. the *a priori* method, which seeks to determine from the kinetic theory of gas what proportion of molecules attain the requisite speed; and the *a posteriori* method, which seeks to ascertain from the observed effects of escape where and on what scale it has actually taken place.

I tried the *a priori* method more than thirty years ago, but had to abandon it, having satisfied myself that *in the present state of our knowledge it cannot be made to furnish a valid investigation.* I came to this conclusion upon grounds which are fully stated in a paper of which the first part will appear in the May number of the *Astrophysical Journal*, and the second and more important part probably in the June number. I then turned to the *a posteriori* method, and endeavoured to develop it in the memoir which Mr. Cook has criticised (see *Scientific Transactions* of the Royal Dublin Society, vol. vi. (1897), p. 305, or *Astrophysical Journal* for January 1898, p. 25).

Both methods, if correctly applied, should lead to the same results: but the *a priori* method, as handled by Mr. Cook and Prof. Bryan, furnishes a different rate of escape from the *a posteriori* method. In such cases there must be a mistake or mistakes somewhere, and in the above-mentioned paper sent to the *Astrophysical Journal* I have endeavoured to trace out where the mistakes are.

The principal errors seem to be three.

The number of molecular speeds which lie between v and $v+dv$

$$=N(\pi+\delta)dv$$

where N is the number of molecular speeds whose distribution is under consideration, π is the probability function (in this case Maxwell's law), and δ may be called the deviation function, as it furnishes the difference which exists between the actual number and that computed by Maxwell's law. In all cases of probability laws the deviation fuction δ is large when N is small; but when the events whose distribution is sought are independent of one another and have causes all of one kind, then δ becomes inconspicuous when N is sufficiently large, and the distribution law may *in such cases* be reduced to $N\pi dv$ without sensible error. This reduction, however, is not always legitimate when, as in gases, the events are so associated with one another and with other agencies that cumulative effects can arise. Then δ may become larger than π in reference to those values of v which make π small. The first omission seems to be the omission to take this into account.

Another omission is the omission to take the size of the element of volume $dxdydz$ into account. This, as experiment shows, may at the bottom of the earth's atmosphere be as small as the cube of one-tenth of a millimetre. But in the regions from which the escape of gas is possible, it has a volume of many cubic miles. This circumstance, which largely increases the opportunity which molecules have of escaping from that situation, has not been taken into account.

But perhaps the most serious error is overlooking the fact that Maxwell's law holds good only of a portion of isotropic gas surrounded by similar gas. That the gas shall be isotropic is one of the data employed by Maxwell in his proof of the law. Another law (which may be, and in fact is, very different from Maxwell's) is the law of distribution of the molecular speeds in a portion of gas as anisotropic as that of the regions from which the actual escape takes effect. The deductions from Maxwell's law may be correctly derived, but the premiss being wrong the conclusion has no significance.

It would be very satisfactory if we had two ways—the *a priori* method as well as the *a posteriori*—of investigating the problem; but with our present limited knowledge of molecular physics, this does not seem to be within our reach.

Mr. Cook at the end of his letter supposes that "the discovery by Ramsay of helium as a constituent of our atmosphere only tends to confirm the results of my (Mr. Cook's) calculations of the impossibility of its escape." This is so far from being the case that the quantitative determinations made in Prof. Ramsay's laboratory are now sufficiently advanced to lead with much increased emphasis to the opposite conclusion. This appears from the following data, which have been generously placed at my disposal by my friend, Prof. Ramsay:—

(1) The proportion by volume of argon in dry atmospheric air is about 1 per cent. of the whole, the volume of neon (to which the present note will not further refer) may be taken as about a thousandth part of the volume of argon, and the volume of helium as about 1/10 to 1/20 of the volume of neon. Accordingly, the volume of helium in dry air is something like from 1/10,000 to 1/20,000 of the volume of argon, or from 1/1,000,000 to 1/2,000,000 of the whole volume of the air.

(2) Both argon and helium are supplied to the atmosphere by hot springs; argon generally by all hot springs which contain atmospheric gases, and helium by some of them.

(3) The argon in such springs, like the oxygen and nitrogen, may be simply gas which had previously been removed from the atmosphere by water. A litre of water under ordinary conditions will absorb about 45 c.c. of the oxygen of the air in contact with it; about 15 c.c. of its nitrogen; about 40 c.c. of its argon; and about 14 c.c. of its helium.[1]

Hence in rain we should expect to find the following proportions preserved:—

$$\frac{20\cdot9}{100}\times 4\cdot5 \text{ of } O_2;$$
$$\frac{78\cdot1}{100}\times 1\cdot5 \text{ of } N_2;$$
$$\frac{1}{100}\times 4 \text{ of A; and}$$
$$\left.\begin{array}{l}\text{from } \frac{1}{1,000,000}\times 1\cdot4\\ \text{to } \frac{1}{2,000,000}\times 1\cdot4\end{array}\right\}\text{ of He.}$$

So far as oxygen, nitrogen and argon are concerned, these proportions are sufficiently nearly those in which the gases are present in the springs referred to. But in those springs in which helium also has been detected, it seems to be present in quantities about 1/10 of the argon—that is, in a quantity which is nearly from 3000 to 6000 more than we can attribute to its having been derived from the atmosphere.

(4) This great excess of helium in some springs has doubtless a mineral origin, some minerals, chiefly uranium compounds, containing much helium which they give up when heated. On the other hand, there does not appear to be any comparable mineral source of argon.

(5) Hence, on the whole, the argon which is being supplied to the atmosphere by hot springs seems to be argon which had previously been withdrawn from the atmosphere and which is being restored to it. Whereas, in contrast to this, there seems to be a continuous transfer of additional helium from the solid earth to the atmosphere always going on.

Thus the facts seem to warrant our inferring:—

(*a*) That the excessively small quantity of helium in the atmosphere is helium on its way outwards.

(*b*) That it would have become a much larger constituent of the atmosphere, by reason of the influx from below, if there had been no simultaneous outflow from above.

(*c*) That the rate of this outflow is presumably equal to the rate of supply; and therefore such as would suffice in a few thousand, or at least in a few million, years to drain away the small stock of helium in the earth's atmosphere, if the source of supply from below could be cut off.[2]

[1] See the determinations made by Herr Estreicher in Prof. Ramsay's laboratory, as recorded in the *Zeitschrift für physikalische Chemie*, vol. xxxi. (1899), p. 184.

[2] If the proportion of helium in the atmosphere is assumed to be something between 1/1,000,000 and 1/2,000,000 of the whole atmosphere (which rather tends to be an over-estimate, since it does not take into account the increased diminution of the density of the helium as it ascends, which is a consequence of its escape from the top of the atmosphere), then the helium in the whole of the earth's atmosphere would, if reduced to standard temperature and pressure, occupy a volume somewhere between a cube of ten miles, and half that space. Now, so far as can be judged from the imperfect observations as yet made on the rate at which helium is being filtered into the atmosphere, it would appear that the present rate of supply is such as would yield this quantity of helium in something like one or two thousand years, and perhaps in a less time.

It thus appears that the recent more exact determinations have raised what was probable when I wrote my memoir into being now almost certain, by showing with greatly increased clearness—

(1) That argon is unable to escape from the earth.

(2) That helium is slowly escaping, and presumably was in a position to escape more freely in the distant past.

It is interesting to observe that another moot question in astronomy seems to be resolved by Prof. Ramsay's work. It is known that the dynamical relation of the vapour of water to Mars is nearly the same as that of helium to the earth. We are accordingly now justified in presuming with greater confidence that water cannot remain upon Mars, that accordingly the polar snows of that planet are probably carbon dioxide, and that some of the other appearances which have been observed are due to the shifting of low-lying fogs of this vapour as they travel alternately towards the two poles.

G. JOHNSTONE STONEY.

8, Upper Hornsey Rise, N., May 20.

"Plotosus canius" and the "Snake-stone."

POSSIBLY the following facts may possess interest for some of your readers:—

A good many years ago, when sea-bathing in the Old Straits of Singapore (*i.e.* those separating the island from the Malay Peninsula), I put my foot in a slight muddy hollow in the sandy sea-bed; the moment I did so, I received an agonising stab near the ankle (from some red-hot poisoned blade, it seemed) which drove me in hot haste ashore, where a Malay constable, on hearing what had happened, and on examining the wound, pronounced my assailant to be the "ikan sĕmbilang" (sĕmbilang fish), *Plotosus canius*, one of the siluroids, I am informed by Mr. Boulenger of the British Museum. The fish is armed with three powerful spines on the head, one projecting perpendicularly from the top, and one projecting horizontally from each side.

The Malay lost no time in running to the barracks near by, whence he shortly returned with a little round charcoal-like stone about the size of a small marble. This he pressed on to the wound, to which it adhered, and remained there by itself, without any continuation of pressure, for a minute or more. Then it fell off, and black blood began to flow, which, after a little, was succeeded by blood of normal colour. The pain, which had been excessively acute, began to diminish soon after this, and in an hour had practically disappeared. The wound gave me no further trouble, but a fortnight afterwards I noticed a hole about the size of a pea where the wound had been.

Another gentleman, who, curiously enough, had suffered in the same way in another part of Singapore the same day, was not so fortunate in his cure, being completely laid up for six weeks.

The black stone applied by the Malay to the wound came, he alleged, from the head of a snake, and claimed, therefore, to be a bezoar stone. It was, no doubt, a snake-stone, probably made of charred bone, and therefore porous in character, which would account for the adhesive and absorptive powers it displayed in my case.

In his "Thanatophidia of India," Sir J. Fayrer (quoted by Yule in "Hobson-Jobson") expresses entire disbelief in the efficacy of these stones as remedies "in the case of the *real bite* of a *deadly* snake," owing to the extreme rapidity with which, in such a case, the venom pervades the system.

However this may be, the late Prof. Faraday, after examination of one of these stones, supplied by Sir Emerson Tennent (quoted by Yule), credits it with certain absorbent powers, and it would seem a pity that the undoubted value of such stones, at all events in minor cases, where they may save a great deal of suffering, should be discredited.

Another remedy, considered of some value by Malays for the stab of *Plotosus canius* is the sap of *Henslowia Lobbiana*, which grows freely on the coasts of the Malay Peninsula.

Among other marine offenders of this class dreaded by Malays are several varieties of the skate or sting-ray, "pari" as they are generically called, and some of the "lĕpu," of which the only dangerous one, I have Mr. Boulenger's authority for saying, is the "lĕpu" proper, viz. *Synancia horrida*. When the skate reaches a large size, he will drag a fisherman's canoe a long way.

Among the Medusæ, one much dreaded is known as "ampai," from its long fringes. The effects, unless a remedy can speedily be found, are painful and trying to a degree, seeming to penetrate the whole frame, as it were, electrically, at once specially affecting the seat of any ailment, and even the teeth and the hair. I have never suffered from it myself, but am enabled to speak to these points from two cases which came under my personal observation. A valuable remedy for this sting, if applied soon, is the juice of the young fruit of the papaw (*Carica papaya*).

A further illustration of the value of some native remedies is supplied by a case which occurred some years ago at Malacca, during my residence there, though I cannot state what the remedies employed were.

A young gentlemen in the office of the Telegraph Company went out to bathe in the sea one night from the end of the pier (in any case rather a rash proceeding, if only for the occasional presence of crocodiles!), when he found himself in the embrace of some creature with long tentacles, from which, after desperate struggles, he eventually succeeded in freeing his legs and his arms, and in regaining the pier. The Colonial surgeon could do nothing for him, and he was in such tortures that for a time he seemed to have lost his mental balance, but nine or ten days after the occurrence a native practitioner, being called in, cured him completely.

D. HERVEY.

The Elms, Aldeburgh, May.

Microphotography, Isophotography, Megaphotography.

I HAVE read with much interest your article on microphotography (p. 4) at its best. Possibly some of your photographer readers may be glad to know that microphotography of sorts is within the reach of all who possess a microscope with suitable substage-condenser and a camera. The results may not compete with the best, but they are very useful. I find that any transparent object which can be conveniently seen in the microscope can be reproduced in the camera. If the fine adjustment is good enough for ordinary work, it is good enough for photographic work.

One of my earliest attempts was to photograph fluid inclusions in quartzes with ordinary sunlight, and rock-sections polarised. The only difficulty was that the sun would not keep still, and without a heliostat the work was most troublesome, not to say aggravating. In one case, a mere movement of the condenser-diaphragm made the bubble in the inclusion fly backwards and forwards. A negative was taken in each position, and a lantern slide taken of each negative. With a little device in the double lantern the motion of bubbles in inclusions can be shown on a nine-foot screen. These negatives were taken with a 1/16th immersion, the camera being extended with a brown paper tube, and the extra apparatus did not cost one shilling.

Up to a ½-inch objective, ordinary gas, with isochromatic plates, does very useful work. The only difficulty to surmount is to handle the focusing apparatus, and see the focusing screen at the same time. A hand mirror solves the problem. But a fine adjustment is really scarcely necessary, as it is easy to focus with the camera as in ordinary photography.

It is often desirable to photograph objects their exact size. Before the Kent's Cavern Collection was divided, I photographed the choicest examples for the Torquay Natural History Society. The implements were fixed with beeswax on a piece of plate-glass, which could be placed in any position and backed by any desired background. I sent a couple of prints to the International Amateur Photographic Exhibition at Vienna, and the jury, much to my surprise, awarded them a diploma. The extra apparatus certainly did not cost 10*s.*, and the negatives were taken in the lecture-room of the Natural History Society under some disadvantages.

Of megaphotography I have but a single experience. While observing the transit of Venus, I thought I would try a photograph. I drilled a hole in the telescope cap for diaphragm; took off the eye-piece and stuffed the telescope into a common camera, with a red cloth to make it light-tight; exposed six negatives with hand exposure on instantaneous plates. Result: four passable negatives and one good one. This quite unlooked-for success was due to some back volumes of NATURE which propped up the camera. The success was really a downright "fluke"; for, knowing the exposure must be hundreds of times too much, I added a quantity of bromide of potassium to the